On the theory of magnetic impurities in integrable correlated electron chains

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In a recent preprint [1] Ge *et al.* reply to our recent response [2] to some concerns raised in [3]. Several obvious remarks are necessary to clarify the situation, because the authors of [1] certainly misunderstand (or misinterpret) some of our statements.

1. In [3] and in [4] the authors imply that integrable magnetic impurities cannot exist in closed $t-J$ and Hubbard chains (cf. P. 795 of [4] and P. 8544 of [3]). It turns out that in [1] they admit that integrable magnetic impurities *can* exist in exactly solvable closed correlated electron chains.

2. The approach (i) in our answer [2] has been named the "quantum inverse scattering method" (also known as the algebraic Bethe ansatz cf. [5]) long before our work on impurities in correlated electron systems, see, e.g., [6]. This standard definition has been used in our reply [2]. The approach (ii) in our reply [2] is known as the "graded quantum inverse scattering method" [7]. Does the strong emphasis in [1] on the difference between the co-ordinate Bethe ansatz and the quantum inverse scattering method imply that the authors of [1] believe that those two methods could yield *different* answers? We are not aware of any such contradictions between the two methods.

3. In [2] we pointed out that the impurity matrix changes the commutation relations in the spin sector (see below). This is absolutely correct, keeping in mind that two parameters, θ and S, which distinguish the impurity site from other sites of the chain are nonzero (note that the impurity scattering matrix used in our papers [8, 9, 10] mixes the states with S and $S+\frac{1}{2}$; this hybridization is sometimes misunderstood).

4. The magnetic impurities we studied in our papers have an essentially different structure than those of [11]; hence, it is no wonder that the solutions do not coincide with ours. Ref. [12] considers the supersymmetric $t-J$ model with a different grading than the

one considered by us and does not consider magnetic impurities. Since this represents a very different situation, it does not contradict our results. Actually, the special case of the impurity of [13] coincides with our results [10].

5. In the approach (ii) for the supersymmetric $t-J$ model the operators $\hat{A}_{12}$, $\hat{A}_{13}$, $\hat{A}_{21}$ and $\hat{A}_{23}$ acting on the vacuum state *do* indeed yield zero (cf. Eq. (3.27) of [7]) in the FFB grading, contrary to the statements in [1]. One can see that the results of [10] for the special case of $\theta = 0$ and spin, equal to the ones of the host, coincide with those of [7] (cf. Eq. (3.50) of [7] and (A1) of [10]). The operators $\hat{A}_{12}$ and $\hat{A}_{21}$ in [1] do not contain any characteristics of the impurity (i.e., θ and S), and are then equivalent to those studied in [7]. This way, the argumentation of [1] can be applied to paper [7], and the criticism presented in [1] actually concerns [7] rather than [10] (in which we essentially used the method developed in [7]). However, the criticism presented in [1] is incorrect, because the authors do not take into account the fact that the eigenvalue of the transfer matrix is determined up to some multiplier [5, 7, 13]. Moreover, the important commutation relations for the spin sector are those between the $\hat{A}_{ij}$ $(i, j = 1, 2)$ and $\hat{A}_{31}$ and $\hat{A}_{32}$ (or $C_{1,2}$, cf. [7]), and those between the latter two operators, which indeed are used as "creation operators" in [10]. Namely, the changes in these commutation relations, but not in those between $\hat{A}_{12}$, $\hat{A}_{13}$, $\hat{A}_{21}$ and $\hat{A}_{23}$ (which are mentioned in [1]), determine the changes in the spin sector of Bethe ansatz equations due to the magnetic impurity.

6. The change of the "class of the representation" (l) implies the change of the symmetry in the considered model (we did not discuss the symmetry of the Lax operator in [2], however, it turns out that the symmetries of our impurity L-operators and those of the host are the same, unlike the case of Refs. [11]). Hence, our statement in response to [3] is correct.

7. Point (5) of our answer to [3] pertains to approach (i), but not to approach (ii). However, in [10] the approach (ii) was used. It is, naturally, correct [1] that in the FFB grading of the approach (ii) one cannot use $\hat{A}_{21}$ as a "raising operator". But the authors of Ref. [1] misunderstand our statements [2] and incorrectly mix the two approaches.

8. Obviously, the statements of our answer [2] (and the results of our previous papers) do not contradict [14].

9. The claim in [1] that Ge *et al.* studied a *spin* impurity, without additional charge degrees of freedom, contradicts the fact that to according their Bethe ansatz equations, e.g., derived in [4], the valence (the occupation number at the impurity site) *varies* with external parameters (such as the chemical potential, a global (non-local) magnetic field), cf. [10]. This is impossible if one studies a pure magnetic impurity, which has only spin degrees of freedom (in this case the valence should be one and not vary with the external parameters, even for $q = 1$).

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